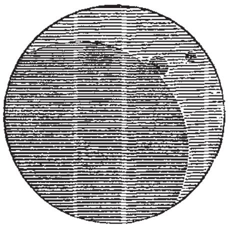
extremely busy, and the ultimate success of our operations must in a great measure be attributed to the unremitting energy of Mr. Lockyer and Prof. Roscoe.

Up till to-day the weather has been superb, day after day just like the warm days we often have in England towards the end of June. The thermometer in the shade has reached from 75° to 80° F., while the barometer has been steady, but with just sufficient tendency downward, to fill with gloomy apprehension the less sanguine of our party. During this afternoon things do not look well, heavy clouds have been sailing over head, and have quite shrouded the upper five thousand feet of Etna; but we have yet hope, and all we can do is to wait patiently for about twenty hours, hoping then to get at least a bright gleam for the space of a minute and a half. If the sky so far favour us doubtless to-morrow will be an epoch in the history of astronomy.

L. Cumming

Catania, Sicily, Dec. 21, 1870

The Eclipse



A remarkable phase in the moon's passage across the sun was the perfect apparent contact of the limb of the moon with a sun-spot, of which the annexed figure is a fair representation. The noticeable thing was that the body of the moon itself and the sun spots were of so precisely the same tint that no trace of a division was perceptible, one appearing to be merged in the other as long as the contact lasted.

Exeter, December 22, 1870 W. F.

Eozoön Canadense

The letter of Mr. T. Mellard Reade on the subject of *Eozoön Canadense*, contained in your number for December 22, exhibits so complete a misapprehension of the state of our knowledge of that fossil, that I feel it necessary to break the silence which I have for some time imposed upon myself as regards this subject, in order that your readers may not be misled by the positiveness of his assertions.

1. Mr. Reade speaks of Eozoön, with the exception of the Tudor specimen, as having been obtained *only from metamorphosed rocks*. In reply to this, I have to state that the Eozoönal structure is *most* characteristically displayed in those portions of the Serpentine Limestone of the Laurentian formation which have undergone the *least* metamorphic change. In fact, the Calcareous lamellæ of the best specimens of Eozoön in my possession show less departure from the shelly texture with which I have become familiarised by the special study of the microscopic appearances of Shell, &c., for more than thirty years, than do the great majority of undoubted shells, corals, &c., contained in the least altered rocks of any geological period.

2. Mr. Reade assumes that the presence of the Serpentine lamellæ, which alternate with the Calcareous lamellæ, is itself an indication of metamorphic action. This position can only be sustained by those who are ignorant of the processes which can be shown to be at present going on upon the sea-bottom, and of which we have evidence in various geological periods; whereby the sarcodic substance of animals of various types of organisation, but especially of *Foraminifera*, undergoes replacement by siliceous compounds precipitated from sea-water during its decomposition. It was long since shown by Prof. Ehrenberg, that green sands of various ages, from the Silurian to the Cretaceous, are essentially formed of the *internal casts* of Foraminifera. The late Prof. Bailey (U.S.) first proved that the production of such internal casts is taking place at the present time. I have long had in my possession a set of beautiful internal casts of this kind, procured from the late Mr. J. Beete Jukes's dredgings on the coast of Australia. And quite recently I have obtained from Captain Spratt's dredgings in the Ægean a most remarkable series of such casts, which includes representations in green and ochreous Silicates, not merely of the sarcode-bodies of *Foraminifera*, but also of the sarcodic network that occupies the interspaces of the calcareous reticulation which I demonstrated twenty-three years ago (Brit. Assoc. Report for 1847) to be the basis of the skeleton throughout the class of *Echinodermata.* And Dr. Duncan has shown that a like process is taking place at the present time in the case of *Corals;* their animal substance being replaced by Silicates, whilst their Calcareous skeleton remains unchanged. No *mechanical* agency can account for this replacement. It is not effected by the percolation of Silicates in solution, under the "*hydrothermal*" action which Mr. Reade (following the lead of Messrs. King and Rowney) invokes as having been concerned in the production of the Canadian Eozoön. And I am justified by the opinion of several of our ablest Chemists and Mineralogists in the assertion that no agency save a progressive *chemical substitution* can account for the production of these wonderful models; the Silicates being precipitated from sea-water by the decomposition of the sarcodic substance which they replace and represent. Whether or not this doctrine be accepted, it may be confidently affirmed that whatever be the agency concerned in *their* production, the filling-up of the cavities of the Calcareous skeleton of Eozoön may be fairly accounted for in the same manner.

3. The most characteristic features of the best-preserved specimens of the Canadian Eozoön can thus be completely paralleled by those of analogous formations at present going on. Let us suppose that the North Atlantic sea-bed, instead of being covered by minute individualised *Globigerinæ*, were occupied by a shell-producing Rhizopod having the indefinite extension of *Bathybius*, and that its sarcodic substance came to be replaced (as in the instances just cited) by Silicates precipitated from sea-water; such a composite formation, elevated so as to become a terrestrial rock, *without any metamorphism whatever*, would be the precise parallel of the *Eozoön Canadense.* And just as, at the present time, the replacing minerals are not always the same, though always compounds of Silica, so the substituted material in Eozoön often consists of other minerals than Serpentine, always, however, being Silicates. In fact it was the *uniformity* of Morphological character, with *variety* of Mineral composition, that first led Sir William Logan—a geologist second to none in experience and judgment—to the suspicion of its organic origin.

4. Mr. Reade represents me as having made "the important admission" that "the several features in the structure of Eozoön (chamber-casts, canal-system, and proper walls) could be separately paralleled elsewhere," meaning, I presume, in undoubtedly Mineral structures. I have nowhere, that I can recollect, made any such admission: on the contrary, I have repeatedly argued that whilst the *combination* of structural characters in Eozoön affords the most unmistakeable evidence to those whose previously acquired knowledge enables them to appreciate their value, there are individual features which are inconsistent with any conceivable hypothesis of its purely Mineral character. Of these I may here state two, which I have always found to be most convincing to such as are familiar with the microscopic appearances of Minerals: *First*, the fact that the "canal-system" which traverses the Calcareous lamellæ passes *across* their cleavage-planes, instead of *between* them; and that this canal-system has precisely the same distribution, whatever may be the mineral which occupies its tubes, whilst its finest ramifications are frequently filled with calcite, as in the least-altered fossil Foraminifera. The idea that such arborescent expansions can have been produced by any kind of *infiltration* of one mineral into another, is thus, in the judgment of some of the most eminent Mineralogists of the day, *altogether untenable;* whilst the *precise parallelism* pointed out by Dr. Dawson, between the canal-system of Eozoön and that which I had shown to exist in the recent *Calcarina*, is no less satisfactory to Naturalists conversant with Foraminiferal structure. *Second*, the fact that the "Nummuline layer" or "proper wall" of the chambers consists of a Calcareous lamella traversed by Siliceous aciculi, which sometimes lie straight and parallel, are sometimes curved, and sometimes penicillate; the precise equivalent to this being shown in the chamber-walls of recent Foraminifera, when the pseudopodia which occupied their tubuli during life have been replaced by Silicates. I assert again, on the authority of Mineralogists of the highest eminence, that such an arrangement cannot be shown in any undoubted mineral, and that it cannot be attributed to any physical agency. To liken this "Nummuline layer" to Chrysotile or any similar modification of Serpentine, shows a misapprehension of its essentially composite structure.

5. I cannot admit that the question of the Organic nature of the Canadian Eozoön (if question it be) is in the least degree affected by the occurrence of Metamorphic rocks presenting more or less morphological resemblance to it, in combination with undoubtedly Mineral characters. We should never think of deci-

© 1871 Nature Publishing Group

ding the nature of Chalk, or of any more ancient Foraminiferal limestone, by the condition of its altered forms; the evidence of their Organic origin being supplied by the microscopic examination of specimens exhibiting the least evidence of change, and this evidence being not in any degree invalidated by the most complete mineralisation of particular examples. A large number of specimens of Ophicalcite have been submitted to me from various sources, as to some of which I have been able to say pretty confidently that they were originally Eozoic, but have been altered by subsequent metamorphism; whilst others do not present any feature whatever which would lead me to assign to them an Eozoic origin. To assume that these last (of which the Strath rock may be an example) are to be placed in the same category with the Canadian Eozoön, and thence to affirm that because they are purely Mineral productions, *it* cannot be Organic, involves a *petitio principii* by which it would be perfectly easy to prove the same thing of Chalk.—Let me illustrate my position by a parallel case. I have lately demonstrated* the existence of a Foraminiferal structure *departing much more widely than Eozoön does from any previously known type*, in a class of globular bodies from one to two and a half inches in diameter, occurring in the Upper Greensand; these having been previously regarded by experienced Geologists as mere Mineral concretions. Now, it so happens that the Magnesian Limestone of the North of England contains large masses of spherical concretionary bodies, bearing a strong *general* resemblance to *Parkeria* in internal structure as well as in outward form, but hitherto regarded, I believe universally, as Inorganic; and a reasoner like Mr. Reade would argue in this way:—"Because impartial geologists have pronounced the Permian concretions undoubtedly inorganic, the Greensand spheres are so likewise; and Dr. Carpenter's *Parkeria* becomes extinct as a fossil." But it likewise happens that the structural features which are most peculiar in *Parkeria* present themselves also in a remarkable living Foraminifer recently obtained from great depths in the *Porcupine* dredgings; so that the truly Foraminiferal nature of *Parkeria* cannot be a matter of the slightest doubt. And the only question now is, whether a careful microscopic examination of the minute structure of the Permian concretions may not afford, through its likeness to that of *Parkeria*, more or less definite indications of *their* Organic origin, obscured by subsequent metamorphism. The application is obvious.

6. I am equally unable to admit that if a rock presenting all the characteristic features of the Canadian Eozoön were to be found shading off into one containing characteristic Liassic fossils, this would afford the least tittle of evidence against the Organic character of the former. As the *Lingula* of the ancient Siluria has, in the judgment of our most eminent Brachiopodist, come down unchanged to the present time, and as even the same *varietal* modifications of Foraminiferal types were existing in the Triassic period as now in the Mediterranean, I see no reason why we should limit Eozoön to the Laurentian epoch. When this subject was last discussed at the Geological Society, I ventured to say that it would not in the least surprise me to find Eozoön, or something very like it, now existing on the deep-sea-bottom; and the notion was not treated by any of the eminent Geologists then present as having any *a priori* improbability. Since that time, the Coccoliths first discovered by Prof. Huxley, and the Coccospheres first observed by Dr. Wallich, in the Globigerina-mud, and afterwards recognised by Mr. Sorby in Chalk, have been detected by Prof. Gümbel in Silurian rocks; so that it is clear that the Biological condition of the deep sea has changed much less in vast periods of Geological time, than has that of shallower waters; whilst the probability has now almost reached a certainty that Rhizopodic life has been at least as largely concerned in the production of Calcareous deposits in earlier Geological periods, as we know it to have been in the later.

7. Though Mr. Reade "feels assured that whenever impartial Geologists take the question up, the fossil itself will become extinct," his assurance is not borne out by the judgment of the large number of impartial Zoolgists, Continental as well as British, who have satisfied themselves, by a careful examination of my series of microscopic preparations, of the Organic nature of the Canadian Eozoön, and have authorised me to express their entire accordance in my interpretation of its phenomena. An eminent Professor in one of our own Universities used this emphatic expression—"The matter seems to me not to admit of *hesitation*, much less of *doubt*." My last Continental visitor, Prof. Carus, who is well known to possess a comprehensive and practical knowledge alike of Zoology, Palæontology, and Mineralogy—assured me that having come without any prepossession on the subject, he left me with a full conviction of the justice of my views. The respect paid by such Naturalists as Professors Milne-Edwards, Carus, Lovén, Van Beneden, and Escher von der Linth,—typical representatives of the Science of France, Germany, Sweden, Belgium, and Switzerland,—to my own judgment in a matter as to which *they* regard the special studies of a third of a century as giving me some claim to authority, may console me for the contemptuous repudiation of microscopic evidence in which Mr. T. Mellard Reade has thought it becoming to indulge. I am far from expecting, however, that anyone should pin his faith upon my own *ipse dixit*, supported though it be by the entire concurrence of my three fellow labourers in Foraminiferal investigation, Messrs. Parker, Rupert Jones, and H. B. Brady. And if it be thought that the decision of any tribunal of really "impartial geologists" is likely to carry more weight with the scientific public than that of the authorities I have cited, I am perfectly willing to go into the question with them; provided, however, that such tribunal consists of, or at any rate includes, men who are sufficiently conversant with the Microscopic appearances of undoubtedly Organic structures, to be able to recognise such appearances when they see them. One of the strongest opponents of the Organic origin of Eozoön designated as "an agatized mineral" a section of a *recent* Nummuline shell, that exhibited a minute tubulation corresponding with that of the nummuline layer of Eozoön, which he had just before characterised in the same manner. Another attributed the production of a perfectly mineralised internal cast of *Polystomella* in green silicate, from Capt. Spratt's Ægean dredgings, to the working-in of mud. And a third has abstained from even looking at my specimens, though I have repeatedly expressed my willingness to give him an opportunity of examining them. Such are *not* the judges before whom I would consent to plead the cause of *Eozoön*.

WILLIAM B. CARPENTER

---

* See my description of *Parkeria* in the Phil. Trans. for 1869.

Mimicry *versus* Hybridisation

ALLOW me space for a word or two in reply to Mr. Wallace and Mr. Butler's observations on my papers on Mimicry and Hybridisation.

There is only one point in my argument to which they have taken exception, and although, of course, I am not therefore entitled to assume that their silence on other points means assent, I may at least infer that in their view the point objected to is most open to assault, and that if it were established, the reader may regard the rest with increased confidence.

The objection is that the instances of hybridisation in plants which I have cited as parallel to the cases of mimicry between the Danaids and Nymphalids were merely cases of hybridisation between species of the same genus or allied genera, whereas these butterflies are more distantly related. The question, as thus put by these gentlemen, resolves itself into a question of comparative degrees of affinity, and Mr. Wallace, with his usual skill, tries to throw the onus of proof from his shoulders to mine. But with all submission we shall keep it where it naturally lies. He puts it that my argument rests on the assumption that hybridisation can take place between different orders or families, and quite logically (supposing me to have done so) objects to my making any such assumption in regard to insects, seeing that nothing of the kind has ever been observed in other animals or in plants. But I rest my argument on no such assumption. I ask no other measure for insects than is given to plants. It is Mr. Wallace who makes the assumption that the amount of difference between Lepidoptera has a different value from that attached to it in any other organic beings. It is he who claims for differences which in any other creatures would be regarded as no more than specific the importance of generic or ordinal. But however this may suit the artificial classification of the systematist, we cannot allow it when we come to deal with the actual workings of nature.

I am not surprised that either Mr. Wallace or Mr. Butler should take what appears to me an exaggerated view of the dignity and position of their favourite group. It is human nature that any subject to the study of which we have devoted ourselves should assume in our eyes larger proportions than it does in the eyes of those who take a wider but less detailed view of it. Hence we see Mr. Butler comparing the Lepidoptera to birds, as if it were a kingdom of equal magnitude, and seeking for

© 1871 Nature Publishing Group